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TO OUR ENGLISH READERS.

We have received from Messrs. Deacon & Co., of 150 Leadenhall street, London, England, a standing order for a large supply of "Science," which will be forwarded weekly. We shall be obliged if our English readers will make this fact known to their friends.

The death of President James A. Garfield is regretted by the nation as a great national loss; but all friends of progress and those who desire to elevate the indifferent and ignorant to a higher grade of civilization, will mourn his sudden death as a calamity; for he was a living example of the wonderful power of education to raise a man from a humble position in society to a post of high honor and usefulness, developing powers which not only opened up a bright and brilliant career, but brought a peaceful and hopeful serenity to his mind which was evident to all who enjoyed his society.

A NEW COMET.

Mr. E. E. Barnard, of Nashville, Tennessee, announced to the Smithsonian Institute, on the 21st instant, the discovery of a comet by him on the 20th, at two o'clock A. M., Washington mean time, in seven hours forty-six minutes right ascension, and thirteen degrees twenty-eight minutes north declension, with a daily motion of three degrees northeast.

On the 23d instant Professor Lewis Swift, of Rochester, made the following announcement in regard to this comet:

The position of Barnard's comet, as telegraphed from Washington, is so widely erroneous that nobody would be able to find it. Instead of being in cancer and having been discovered at two o'clock in the morning, it was near zeta virginis, low down in the Western horizon, and can be seen but a few minutes. It was discovered on the evening of the 19th, and at 7h. 46m., Washington mean time, of the 20th, was in right ascension 13h. 28m. 2s., declination north 3 deg. 47 min., with a daily motion of 3 degrees northeast. In consequence of smoke I have not been able to find it.

We trust in our next issue to offer some explanation of these contradictory statements.

One of the most interesting and valuable reports that has been issued by the Board of Education at Washington, is that recently printed, which describes the opportunities for instruction in Chemistry and Physics which at present exists in the United States, together with statistical tables relating to this subject.*

The Department was fortunate in securing the services of Professor F. W. Clarke, Professor of Chemistry and Physics in the University of Cincinnati, to draw up this report, based on the mass of facts and figures bearing on this matter, which had been collected in reply to circulars issued by the Commissioner of Education towards the close of the year 1878. Professor Clarke appears to possess both executive and literary ability of a high order, and being himself a chemist and a teacher of science, was clearly in a position to do justice to the excellent intentions of Commissioner Eaton. We congratulate Professor Clarke on his success in compiling the technical part of his report, and we propose, on this occasion, to refer to some of his critical remarks aud suggestions, which, in scientific circles, will be considered the most valuable result of this investigation.

Before discussing the condition of scientific instruction in public schools, it may be well to consider first, at what age such instruction shall be commenced, and whether it should be considered as a part of primary education, or be reserved for high schools and universities, where special courses of training in the various branches can be advantageously advanced.

Professor Clarke claims that oral instruction in chemistry and physics can be made intelligible to children of ten years of age. He admits, however, that there is a tendency towards over-cramming the lower schools with a too great variety of subjects, which lead to results which are undesirable. He therefore suggests a compromise, and proposes, that in primary schools a taste for science should be cultivated among children "through the medium of the reading books, which might properly contain some short extracts relating to natural science." This plan Professor Clarke considers would be beneficial, and could not be injurious.

We can find no objection to such a course, provided a suitable reading book be written for the purpose, but before any discussion can be made as to the propriety of teaching the sciences in any form in the primary schools, a more thorough reform in the

*Circulars of Information of the Bureau of Education No. 6. 1881.
A report on the teaching of Chemistry and Physics in the United States, by Frank Wigglesworth Clarke, S. B., Professor of Chemistry and Physics in the University of Cincinnati. Washington, 1881.

whole system of public school education must be made. It appears superfluous to urge the teaching of science to children of ten years of age in public schools, where the elder scholars of fourteen and fifteen years are unable to read aloud intelligently or write an ordinary letter legibly. And yet, to our knowledge, such is the situation of too many of the scholars in our public schools in regard to these fundamental branches of education.

Professor Clarke's report shows that in high schools and academies the teaching of Chemistry and Physics varies between widely separated limits; in the majority of cases mere text book work is done, and only a few experiments performed by the teacher.

In our opinion the report offers excellent advice in regard to the teaching of Chemistry and Physics to such classes of students.

The following extract will explain Professor Clarke's views:

"That chemistry and physics are desirable branches to teach in schools of the grade now under discussion is pretty generally admitted, although a few educators still hold that such studies are fit only for technological institutes and colleges. But the greater number of pupils cannot go out into these higher grades, and must therefore either study the sciences now or do without them altogether. The latter alternative is clearly the wrong one to choose; at least, if we admit that education is anything other than a mere system of mental gymnastics. If subjects are to be learned quite independently of their relations to active life, then there is no ground for present argument; but if culture and utility are both to be considered we must recognize that some scientific training is indispensable. Nearly every pupil goes out of school into one of the great industries; and, whether he becomes a mechanic, manufacturer, railroad man, telegraph operator, farmer, miner, or tradesman, he is likely to encounter practical applications of the two sciences. In every avocation some knowledge of either physics or chemistry is almost certain to be directly useful; and this utility is often so great that the schools can better afford to err on the side of over-thorough teaching than in the opposite direction."

In answer to the inquiry how far these sciences can be carried in such schools without detriment to other interests, the report states:

"One high school has three years and another four years in its total course of study; the latter is plainly able to give more time to any particular subject than the former, Every variation in the character of a school must involve corresponding variations in the treatment of these two sciences. It may be safe to put half an academic year as the minimum time assignable to either subject. A year can usually be given to each without difficulty."

While as to the detail of such instruction Professor Clarke says:

"Instruction should be general rather than special. The attempt is too often made to teach applied science when there are no foundations of science to apply. Such foundations should be thoroughly laid in the high schools and academies, so that the pupil who passes on to a university or polytechnic course may have a genuine preparation for advanced work. Fundamental ideas, like those of the conservation of energy, the correlation of forces, the conceptions of atoms and molecules, &c., ought to be clearly inculcated. The scholar should be made to realize that each science is a coherent whole with definite relations to other sciences, that all its parts are vitally connected, and that certain general principles are universally applicable in all of its branches. In chemistry it is better to concentrate all efforts upon the inorganic portion of the science, leaving the complicated organic side for more advanced study. Along with the merely descriptive work should go a solid drill in chemical problems and chemical notation. Experiments made before classes ought to bear as far as possible upon main questions, and unavoidable details should be handled so as to illustrate clearly the great central ideas. When these have been fairly grasped, the scholar has gained something of both practical and intellectual value. His studies will have brought him not knowledge only, but also increased power."

For success, much depends on the teacher.

"He must have a vivid sense of what needs to be accomplished and enough special knowledge to render him in a measure independent of text books."

Text books, the report says:

"May be useful or injurious, according to circumstances. If they have been chosen by an average school committee, influenced by some publisher more energetic than his rivals, they are likely to be worthless, and the teacher must be prepared to make good their omissions and correct their blunders. No text book can be taken as sole guide and followed without variation; but a good treatise upon either science, prepared, not by a professional school-book maker, but by a trained specialist, may be of great help to teacher and pupils."

Professor Clarke wisely urges the value of laboratory work:

"In addition to classroom drill, laboratory practice should be an essential and prominent feature of every chemical or physical course. In the recitation or lecture, general principles are taught; in the laboratory, the student becomes familiar with methods and details. Three months of laboratory work will give more real insight into any science than a whole year's study of the printed page. To study chemistry from books alone is like learning a language from its grammar only, without attempting to translate or to write exercises. The pupil must learn to observe and to experiment for himself, in order to acquire any clear scientific knowledge."

One recommendation of the report is strongly in accord with our own views, and that relates to the advice to teachers and pupils, to construct as far as is practicable all apparatus used in the laboratory.

"The apparatus which a teacher contrives for himself

with the aid of his scholars is oftentimes the most useful for purposes of instruction. Many and many a school has invested in trifling electrical playthings a sum of money which would have gone far towards the establishment of a simple working laboratory.

"In physics the laboratory practice must needs be somewhat limited. The pupils may handle whatever apparatus happens to be available, learn its manipulation, and assist the teacher in the construction of simple appliances. The magnetization of needles, the electrolysis of liquids, the verification of the fixed points upon a thermometer, and rough determinations of specific gravity, boiling point, and melting point are among the many experiments which ought always to be possible.

"In the chemical laboratory a much greater variety of work is easily attainable. There are the ordinary experiments in manipulation, such as the bending of glass tubes, filtration, precipitation, distillation, &c., the preparation of the commoner gases, acids, and salts; the verification of the more obvious properties of the chemical elements; and lastly, the simpler reactions of qualitative analysis. To the last named subject some time may always be profitably assigned. No other class of exercises will do so much towards impressing the average beginner or towards making him realize the nature of chemical reasoning. At every step it calls his powers of judgment into play. It involves the use of no costly apparatus, and enough can be done for all school purposes with a very moderate supply of the cheaper chemicals. At an expense of a hundred dollars a year a great deal can be accomplished; and an outlay of only one-fifth of that sum may yield results which are by no means to be reckoned as trivial. Again let it be said that success depends upon the teacher and not upon the cost of materials."

We shall in our next issue continue our notice of this interesting report.

ON COMETARY APPEARANCES.[1]

By M. Jamin.

[Translated from the French by the Marchioness Clara Lanza.]

The question of comets seems at present to occupy the attention of all *savants*, and as M. Faye has prevailed upon physicists to take up the subject also, I have decided to enter into the discussion, not with the intention of creating any novel hypothesis, but rather to oppose that which M. Faye imagines to be the correct one. In the first place, it appears to me unnecessary. It contradicts in my opinion, the theory of the vibration of the ether. Besides, it deprives the law of gravitation of its generality and simplicity. In his first work, M. Roche determined, by means of calculation, the form of the horizontal strata of cometary atmospheres subject to the sun's attraction, but he omitted to note the variations of temperature occasioned by the solar rays on the two sides of the comet. In this way he was led to think that the latter must have two tails, one turned towards the sun, the other away from it, which supposition is contrary to reality, as it should be, in fact, inasmuch as it overlooks the cause which manifestly determines the unsymmetrical forms of the two sides. In a second paper, however, he makes a correction, by supposing the existence of a repulsive force diminishing the solar attraction about 1—to 1. φ, φ, being a force acting unequally upon different substances, and which is in reverse ratio to their density. This hypothesis admits of the calculation being achieved with facility, but it has no physical actuality. It is confined to replacing the warmth of the cometary atmosphere, which should be included in the calculations, but which has been forgotten, with a wholly imaginary action whose existence no experiments have ever confirmed. I shall endeavor to re-establish the effect due to the unequal warmth of the two sides by referring to analogous conditions which should exist between the Earth and comets.

Upon the Earth, every day throughout the year, the solar rays one after the other in regular succession strike normally all the points of a circle perpendicular to the axis of rotation and near the equator. These points on all portions of the globe are those that receive the maximum of heat at noon. They constitute what is termed the *ring of aspiration*. The air there really becomes rarified and ascends, advancing towards the north or the south, as the case may be, determining two gaseous currents called trade winds. These currents are permanent and regular; they come from temperate climates, grow warmed progressively in their course, carry with them an intense evaporation, are slightly deviated towards the west in consequence of the Earth's rotation, and finally meet obliquely upon the ring, to rise to the highest atmospheric limits. There they spread, then taking a contrary course, return, one towards the north, the other towards the south. These are the *counter trade winds*. There are, therefore, on the two sides of the ring of aspiration, two closed atmospheric currents completely enveloping the globe, coming cold from the poles, grazing the Earth, and then returning warm, by a higher route. There is no occasion to dwell upon the chief *rôle* played by this circulation. It is sufficient to merely demonstrate its necessity, its constancy and its extent, besides recalling the theory due to the famous Halley, which has never been contested.

This circulation would still exist although under changed conditions, if the Earth instead of turning on its axis always presented the same side to the Sun. The ring of aspiration would be reduced to a single point, the trade winds would converge in all directions, while the counter winds would diverge in the same way. All points of the Earth would send to this summit cold air which would grow warm there, *rise in the form of a cone toward the Sun, spread, become bent upon the edges like the chalice of a cyrathiform flower*, leave the Sun by a high route and after a more or less prolonged journey would return to the point of departure grazing the Earth's surface. It is very evident that this double movement would possess an increase of force proportionate to the Earth's approach to the Sun; that its atmosphere would be more extended, and that there would be a greater mass of water to be evaporated. This does not imply any particular repulsive force.

But let us get to the comets. In the long journey which they perform slowly until they are beyond the Solar world, they have plenty of time to lose all the heat received from the Sun, and to efface all traces of perturbation. The tail disappears, the matter is knit together by its own attraction and assumes a nebulous, spherical form. In the centre are the dense, solid substances, the nucleus, then the liquids and finally the gases. An enormous atmosphere and a very small nucleus. In Donati's comet the nucleus measured 1600 km., while the atmosphere was 20,000 km. The comet of 1881 was still more extraordinary. Its aureole measured 2,000,000 km., the nucleus was reduced to 680. This is just contrary to the Earth whose diameter amounts to 12,000 km., while its atmosphere is merely a thin pellicle of 18 or 20 leagues. Comets are so constituted that the most tremendous atmospheric movements are developed be-

[1] See *Comptes Rendus*, August 16, 1881.

neath the Solar action incomparably more accentuated than those exhibited by the Earth.

As no rotatory motion has ever been observed in comets or their atmospheres, we feel authorized in saying that if it does exist it is exceedingly slow, without taking into account the fact that comets always present the same side to the Sun. The second method of heating should therefore be produced. In every plain passing through the centre of the Sun and the nucleus, there will be a double atmospheric circulation. On the interior, the comets advance towards the Sun as though the gravitation there was intensified. On the exterior, they deviate as if the gravitation was diminished, or rather as though there existed some repulsive force emanating from the Sun, affecting the exterior surface of the cometary atmosphere, and acting solely upon it. In reality, this repulsive force does not exist. It seems as though it did, however, and under conditions analagous to those inferred by M. Faye. All the consequences therefore, which he deduced in order to explain the formation of the tails, are developed naturally. There is nothing here to be altered.

I do not think, however, that this theory is sufficient to account for cometary appearances. On the contrary, it is my opinion that electricity has a great deal to do with them. But before entering upon this let us first return to terrestial phenomena.

It has been satisfactorily proved that considerable electricity exists in atmospheric altitudes, and that it increases according to the height. It is admitted generally that atmospheric motion results; that it is developed by evaporation at the ring of aspiration; that it moves from the time it leaves this ring until it reaches the poles under the form of two currents in the rarified air which it illumines. Towards the sun it is the zodiacal light, invisible when close to this planet, but extending a sufficient distance to be perceived, especially near the equator. Close to the poles it is the aurora borealis, which we see obliquely and which appears more luminous than at the zenith, because it has greater density and is more concentrated.

Upon a comet the warmth occurs at the point where the trade winds come together opposite to the sun. But analogous electric actions should be manifested, illuminate the head and produce the appearance of effluviums succeeding each other like the stratifications in a Geissler tube, accompany the counter trade winds to the opposite side to illumine the tail, and be prolonged to a great distance like the luminous rays in Mr. Crookes's apparatuses. No doubt, matter would be contained in the tail, but rarified to an extreme degree and made visible by both the solar light and the electric current.

M. Flammarion would be quite right then to attribute this shining to electricity. On the other hand, M. Berthelot's observation would be justified, and the development of this electricity would be due to the phenomena of evaporation and movement situated in the atmosphere. We must insist upon this point.

The recent study of cometary spectra has shown us beyond the possibility of a doubt that the interior aureole and the tail contain carburetted gases which emit a light of their own. Now, they can only become luminous in two ways; either by combustion or by an electric effluvium. If by combustion, we have yet to explain how they take fire and how they continue to burn indefinitely, which seems very difficult. For in this case, all the materials of which the comet is composed would be red, and the spectrum would contain the bright spectral rays of the metals as we see them in the electric arc burning n mid air. Nothing of this kind occurs. The light is absolutely like that of the arc when the vaporous carbon is transported to the torpid gases without burning. It shows no brilliant metallic bands, any more than this arc. The light, therefore, cannot be the result of fire, but is due to illumination made by the currents.

I think that the Sun determines gaseous currents in cometary atmospheres analogous to terrestrial trade winds and counter trade winds; that this circulation produces near the Sun effluviums arising from the head of the nucleus and transports to the opposite side the substances which are on the exterior, producing upon these substances the effect of a repulsive force emanating from the Sun, a force which has absolutely no *raison d'être*. Besides this, I think this circulation is accompanied by an electric movement which illumines the gases either towards the head or tail, as the case may be, making them visible to us notwithstanding the feebleness of their density, and precisely on account of this feebleness.

AMYLOSE: ITS CONSTITUENTS AND METHODS FOR THEIR ESTIMATION.

By H. W. Wiley, Lafayette, Indiana.

I propose the name Amylose for all the varieties of sugar and sugar-like substances derived from starch.

These substances are now known by many different appellations, and often the indiscriminate use of these terms gives rise to a great deal of misunderstanding and confusion. Among them I may mention grape sugar, starch sugar, dextrose, dextrine, glucose, maltose, fruit sugar, etc. These names do not always have the same signification in different localities. For instance, glucose and dextrose, in Europe, signify the same product, while in this country they embrace many other substances besides.

If we designate the starch sugar in general by amylose then the terms glucose, dextrose and maltose can be used to designate certain definite constituents of amylose.

Amylose is composed of three principal ingredients.

1st. Dextrine. Pure dextrine is very difficult to obtain. It is obtained almost pure by the dry roasting of starch. The temperature during torrefaction must not be carried too high, 210°–275°. Starch itself has a specific rotatory power of 214° (1). Bondonneau (*loc. cit.*), asserts that there are three dextrines, (α), in which aj (2) = 186°; (β), in which aj = 176°; (γ), in which aj = 164°. According to Musculus and Grubber (3), there are five dextrines; viz.: (*a*), soluble starch colored wine red by iodine aj = 218; (*b*), Erythro dextrine, red color with iodine; rotating power not given.

(*c*), α Achroodextrine, not colored by iodine, j = 210.

(*d*), β Achroodextrine, aj = 190.

(*e*), γ Achroodextrine, aj = 150.

Of these varieties the first and second do not reduce the alkaline copper solutions while the others do. If the reducing power of dextrose be taken at 100, that of the third of the above dextrines will be 12; the fourth 12 and the fifth 28.

O'Sullivan admits the existence of but one dextrine with a = 214.

Thomsen (4) tries to show by history of multiples in the rotating power of the carbo-hydrates that there are at least three dextrines in which the value of aj is 186, 176 and 164 respectively.

I will not multiply authorities concerning the rotating power of dextrine. I have quoted enough to show the highly chaotic state of our knowledge on the subject.

The chemical properties of dextrine are equally as undeterminable.

Gentele (5) is quite confident that dextrine will reduce the alkaline copper solutions and on this he bases his method of separating dextrine from other reducing substances by ferricyanide of potassium.

Stommer (6), Bondonneau (7) and Rumpff (8), are equally

(1) Bondonneau, Ber. d. Deu. Chem. Gesel., IX, 69.
(2) a = specific rotatory power.
(3) Comptes Rendus, LXXXVI, 1459.
(4) Ber. d. Deutsch. Chem. Gesel., 14—2—158.
(5) Ding. Journal, CLII, p. 139.
(6) " " CLVIII, p. 40.
(7) Bull. de la Soc. Chim., 1874, XXI, p. 50.
(8) Zeit. fur Anal. Chem., 1870, p. 358.

positive that dextrin does not reduce the copper solution except on prolonged boiling.

My own experience is that dextrine does not reduce Fehling's solution except on prolonged boiling and then by the dextrine being slowly converted into dextrose.

To secure the reduction of the dextrose and the reducible portion of maltose it is not necessary to boil the solution more than two minutes, and during this time the amount of dextrine reduced is wholly inappreciable.

Color with Iodine. Bondonneau (9) says, dextrine colors a solution of iodine a dark red, but that it is without effect on caustic soda. Musculus & Gruber (9), found that some varieties of dextrine colored iodine solution red, while others were without action on it.

In general it will be found that most substances purporting to be dextrine give a reddish color with iodine. For my part I do not know of any method by which absolutely pure dextrine can be prepared.

Fermentability. Respecting this property the most contradictory statements are found. The weight of authority leans to the non-fermentible doctrine. It is probable that before dextrine ferments it is first converted into dextrose.

Formation. In addition to the method by roasting, dextrine is always formed when diastase or dilute acids act on starch.

Some authorities maintain that dextrine in such cases is always the first transformation product and that the others are derived from it.

According to Musculus (10), dextrose and dextrin are formed synchronously by the action of sulphuric acid on starch and in the proportions of one part of the former to two of the latter. This proportion is maintained until all the starch disappears. The further action of the acid then tends to convert the dextrin into dextrose. O'Sullivan (11) states that by the action of diastase, both dextrine and maltose, and nothing else, are first produced from starch under certain conditions of temperature (below 63°), and in the proportions of one part of the former to two of the latter. The proportion, however, only obtains when the specific rotatory power of the mixed bodies is equal to 171°.

From a careful study of all the data I could obtain I concluded that very little is yet known of the real proportions of dextrine which amyloses formed by heating starch with dilute acids contain.

MALTOSE. Dubranfaut (12) first pointed out the probable existence of a third transformation product of starch in addition to the two which had long been known. But we must accord to O'Sullivan (13) the merit of having first isolated and studied the properties of this body. He has given in the *Journal of the Chemical Society*, within the last decade several papers on the properties of this important saccharide. He gives its specific rotatory power (a)= 150°. The same value is also given by Musculus and Gruber, (14),

Joshida[15] found a slightly higher number. For (a)j this would give 135.°36.

Maltose is formed chiefly by the action of diastase on starch at a temperature not exceeding 75°. But it is also formed, but in smaller quantities by the action of dilute acids.

It is well established that maltose has the power of reducing the alkaline copper solution and in the proportion of 65 to 100 compared with dextrose. We here find an easy explanation of the fact that so many chemists have affirmed that dextrine acted on copper solutions. In all these cases the dextrine doubtless contained maltose. Without citing further from the literature of maltose, which is all recent, I wish to call your attention again to the numbers representing its rotating and reducing power, viz. 150 and 65.

3d. DEXTROSE. This substance is the final product of a complete saccharification of starch. It has a slightly bitter taste, which is probably due to the development of a bitter principle on long boiling with acids. This bitter taste is not noticed in the products less perfectly converted.

Dextrose possesses in the highest degree the power of reducing the copper solutions. One gramme reduces 2.205 g. weighed as cupric oxide. Its specific rotatory power has been the subject of much controversy.

O'Sullivan gives 57°.6 = (a)
Tollens " 56 = "
" " 53.17 = (a) j. (16).

The differences in results which the above numbers show do not indicate so much errors of observation as they do the impurities which the purest dextrose is likely to contain. As a mean of these numbers, we may take (a) = 56 and (a) j. = 50.5 (by calculation).

Pure dextrose is almost insoluble in absolute alcohol, while it is very soluble in water. According to Anthon (17) 100 parts water dissolve 81.68 parts dextrose. To dissolve one part, 50 parts of alcohol, .83 sp. gr. are required. When subjected to fermentation, dextrose affords 48 per cent alcohol (18), while cane sugar and maltose each gives 51 per cent and dextrine none at all.

I have thought it useful to give the above brief ***resume*** of the literature of amylose, because the conclusions to be drawn from it will go far to explain the anomalies of the numbers which the following analyses and methods of analysis will show. The whole subject is a matter of considerable scientific interest on account of the immense production of amylose in this country and the laws which some of the States have passed to regulate its sale. In the present state of our knowledge I am at a loss to see how the real constitution of an amylose, or mixed sugar, can be established before a court of justice. To show this I will give synopses of a few of the

METHODS OF ANALYSIS

which have been proposed.

I shall not attempt to give an out-line of all the methods which have been proposed for determining the amount of dextrine and dextrose in amylose. Until within a few years they were all based upon the assumption that these were the only transformation products of starch—an assumption which we know to be false. Reduced to first principles, all the methods may be comprised under three heads.

1st. The reduction of certain metallic salts by the dextrose, and estimation of dextrine by difference.

2d. Fermentation of dextrose and estimation of dextrine by difference.

3rd. Precipitation of the dextrine by strong alcohol and estimation of the dextrose by difference.

Remembering the facts established in the first part of this paper, it will not be hard to show the fallacies of these several methods.

1st. Metallic salts, especially the compounds of copper, mercury and ferro-cyonigen, are all reduced by maltose as well as by dextrose, and on prolonged boiling in a slight degree by dextrine also. Thus the total reducing effect does not measure the amount of dextrose present only in case maltose is completely absent. In commercial amyloses this is never the case unless it be in rare instances of high pressure conversion.

2d. Fermentation not only converts dextrose into alcohol and carbonic dioxide, but acts in the same manner on maltose. For a given weight maltose gives even

(9) Loc. cit.
(10) Annal. de Chem. et Phys., p. 203.
(11) *Chemical News*, 1876, 861-218.
(12). Anall. de Chem. et Phys. (3) XXI, p. 168.
(13). Jour. Chem. Soc. Vol. X, 2d series.
(14). Loc. cit.
(15). Chem. News, 1881.

(16). Ber. d. Deutsh Chem. Gesel., 1876, p. 420.
(17). (*Ding. Journal*, CLV, p. 41).
(18). O'Sullivan loc. cit.

a higher percentage of alcohol than dextrose, and in the proportion of 51 to 48. Whether dextrine is fermentible or not has long been a subject of bitter discussion. The weight of authority seems to be in favor of its fermentability by a slow conversion into dextrose. Thus by fermentation we are not sure of getting the amount of dextrose present even when maltose is absent. From this it appears that the process of fermentation is likely to give less reliable results than reduction affords.

3d. Precipitation with alcohol is even less reliable than the method just given. Alcohol of 90 per cent ceases to give a precipitate long before the dextrine is all converted into dextrose, as every grape sugar maker well knows. If absolute alcohol is used, dextrose is also precipitated. Anthon has shown[19] that fifty parts of alcohol of .83 sp. gr. will only dissolve 1 part of dextrose. Anything like an accurate separation, therefore, by this method is impossible.

As an illustration of the variations in the composition of different specimens of amylose I will cite the following analyses by Steiner[20]:

	I.	II.	III.	IV.
Water	15.50	6.00	13.30	7.60
Ash	.30	2.50	.40	1.10
Dextrose	45.40	26.50	76.00	...
Dextrine	9.30	15.90	....	39 80
Maltose	28.00	40.30	5.00	42.60
Carbo-hydrates	1.50	7.00	5.30	8.90

My own analyses have given results quite as puzzling as the above, although I have never been able to satisfy myself with such exact expressions of percents.

I have heretofore been glad when, by hard work and liberal guessing, I could come any way near the truth. The author of the above table leaves us in charming ignorance of the methods by which such accurate percents were obtained; at least, of methods which would stand the test of criticisms, while the bunching of all the unknowables as *carbo hydrates* is quite worthy of the reasoning of the Concord School of Philosophy. I have given enough, I think, to show the untrustworthiness of methods now in use, and of the results obtained by them. I am sorry that I have nothing very much, if any better, to propose as a substitute. What I have been able to accomplish I will now briefly describe.

THE ANALYSIS OF AMYLOSE.

Water. I have estimated water in a flat platinum dish. Only two or three grammes should be taken, though in many of my analyses I have used more. This dish is placed in a second one, and this in a paraffine bath heated to 150°–170°. The object of the second dish is to keep the wax from touching the dish which is to be weighed. After two hours the weight is sensibly constant, and the whole mass is quite brown. I believe the method will give the water to within one-half of one per cent.

Ash. There is only one method of determining the ash, *i. e.*, by incineration in a shallow platinum dish in a muffle. The per cent of ash in a strait amylose is extremely small. In most cases its quantity may be neglected as far as practical purposes are concerned. The determination of the ash is chiefly useful to furnish a clue to the purity of the sample.

Reducing Matter. I determine by Fehling's solution. It will be found most convenient to take 10 *g.* in 1000 *c. c.* In all cases the volumes of the solutions employed should be as nearly the same as possible.

Rotating Power is determined by using 10 *g.* of the amylose in 100 *c. c.* If 26,048 *g.* are taken the weight of cane sugar, which gives 100 divisions on most polariscopes, the rotation is so great that the neutral point is thrown entirely beyond the graduation. In many cases of high conversion, however, this would not be the case. If the solution is turbid it must be classified by blood charcoal, or plumbic acetate. These substances, as I have shown ([21]), tend to diminish the rotating power. The clearer definition, however, of the neutral point in part compensates for this loss in gyratory power.

[20] Zeitsch. f. d. Gesch. Brauwesen, 1879, No. 11, p. 339.
(21). (These proceedings Vol. 28, p. 317.)

In the light of the foregoing *resumé* it is possible to explain the results of my own work, although I am far from thinking that anything better than approximate per cents can yet be obtained.

We have seen that an ordinary amylose, whether liquid or solid, contains about 86 per cent of material not water. One per cent, nearly, of this is ash and optically non-active matter. Ten grammes of amylose, therefore, will contain an average of 8.5 grammes optically active matter. If this were all dextrose it would give in 100 *c.c.* an angular rotation of 8°.67. This is obtained by the formula $a = \dfrac{\theta \times v}{\lambda \times w.}$

Here a_j = sp. rot. power for yellow ray.
θ = angular rotation.
V = volume of solution in *c. c.*
λ = length of observation tube.
W = weight of substance in grammes.

In a large number of cases where I heated the amylose with dilute sulphuric acid from 4 to 6 hours, I obtained an average value of $\theta = 8°.85$. This shows that prolonged boiling does not convert all the amylose into homogeneous dextrose.

If the substance under examination were all maltose the value of a_j would be 135.36 and θ would become 23° nearly. The highest number I ever obtained for an amylose was 22° .24. This shows that even this specimen, with such a high rotating power contained some matter in a weaker degree of optical activity.

Finally, if all the substances present were dextrine, I would not be able to tell theoretically what its rotating power would be, since we have just seen that dextrine is assigned different degrees of activity by different authors. As a mean of these I think we may place dextrine $a_j = 176°$, although I do not wish to be understood as stating its real value.

This would give a total angular deflection of 29° nearly.

The problem of analysis is therefore at the present time in the following *status:*

1. In every amylose there are present at least three kinds of optically active matter, viz., dextrose, maltose and dextrine.

2. There are present in every amylose two kinds of reducing matter, viz., dextrose and maltose.

3. A high reducing power shows a high percentage of dextrose present.

4. A high rotating power, which is always shown when the reducing power is low, indicates a large percentage of maltose and dextrine.

5. From a very extended series of analyses, I will say that there is no method known which will give reliable, or rather exact, numbers for the percentage of the different constituents.

6. I propose to attempt the accomplishment of this very desirable result by first polarizing and then reducing the sample, and then repolarizing the residue. The difficulties of preserving a standard volume and of getting a solution sufficiently clear for polarization have prevented me hitherto from obtaining any results. I hope to overcome these troubles and to establish thereby a reliable optical method of determining the percentages of dextrose, maltose and dextrine in amylose.

Prof. Cantoni has been appointed director of the meteorological observatory, to be erected at Pavia. Observations are to be made on the influences of light, heat, and electricity upon vegatable growth, in addition to the ordinary meteorological and magnetical work.

[19] Ding. Jour., CLV., p. 41.

A REMARKABLE INVASION OF NORTHERN NEW YORK, BY A PYRALID INSECT:*

Crambus vulgivagellus.

(Abstract.)

BY J. A. LINTNER.

About the middle of May, of the present year (1881), a serious invasion of St. Lawrence county, N. Y., and several of the adjoining counties, by the "Army Worm," was announced by the newspapers, and by letters addressed to me. It was stated that many pastures had been completely ruined, and the entire destruction of the pastures and meadows was feared. I had never witnessed the operation of the Army Worm—comparatively rare in the State of New York—and I at once visited the infested locality for personal observations.

The reports had not been exaggerated. The ravages were widespread and serious, already extending over eight of the northern counties. Hundreds of acres of grass presented a brown appearance, as if the grass had been winter-killed. A pasture lot of fifty acres, examined by me, which ten days before offered good pasturage, was now entirely brown from the complete destruction of the grass—so thorough, that in places not a blade could be discerned in an area of a square yard, by careful search. Numerous dead caterpillars were adhering to the dried stems of last year's grass, which, it is believed, had fallen victims to starvation.

Several interesting features characterized this attack. It appeared first on upland pastures, differing in this respect from the invasions of the Army Worm, *Leucania unipuncta.*

The progress was remarkably rapid. A browned patch would rapidly extend its area, until it overspread the entire field in ten or twelve days. It could not be ascertained if this was by the spread of the larvæ from certain points, or from the unequal hatching of the eggs, uniformly distributed over the field, as influenced by various conditions. The secresy of the depredation was unusual. The larvæ had seldom been seen and never observed in active feeding. It was believed that they fed at night, by drawing in the blades of grass to their subterranean retreats.

In two instances the larvæ were observed in immense numbers, collected on trunks of trees—so numerous that they could have been scooped up by handsfull. One of the reported localities was visited by me where the assemblage had been noticed three days before. The tree-trunk, at its base, was found to be enveloped by a web of silk, as was also an adjoining stump, of so firm a consistence that it could be lifted up in a sheet, like a piece of woven silk. The cause of the congregation at this point could only be conjectured, but it was thought it might have been for shade, after the complete destruction of the surrounding pastures. It was not for feeding on the foliage, for the grasses were alone eaten by the larvæ.

It was generally accepted throughout the entire region as an army-worm invasion, and the most disastrous consequences apprehended. The papers abounded with notices of it. Farmers commenced to dispose of their cattle, in the prospect of their ruined pastures and meadows. It became the one topic of village conversation, and general alarm prevailed.

The caterpillars observed and collected by me in Morley and Potsdam, by digging in the soil, and occasionally finding one on the surface, were slender, cylindrical, sixteen-footed, of a sordid or obscure greenish color, with a shiny black head. They were destitute of lines or other ornamentation than some verrucose spots on the dorsal portion. The average length was three-fourths of an inch.

I was unable to identify these with *Luecania unipuncta*, for they were quite unlike the mature form of that species which I had alone seen. Yet it was possible that they had additional moltings to undergo, which might result in a material change of appearance. Their habits seemed to be quite different from those of the army-worm, and it was nearly two months too early for an invasion of that species.

Of the larvæ which I had brought from Potsdam for rearing and ascertaining the species, nearly all died shortly thereafter. Only a single one developed, giving me a small Pyralid moth—*Crambus exsiccatus.* Additional ones were sent me, at my request, from Potsdam. They were quite different from those previously collected and observed by me, but it was believed by my correspondent, as the result of observations made, that the molting through which they had just passed had produced this change.

I suspected that two species were associated in the attack, but other pressing duties at the time prevented a decision upon this subject. Some of the examples received were submitted to Prof. Riley, who was able to identify them as the larvæ of a rather rare Noctuid—*Nephelodes violans*, which he had known in Missouri. The occurrence of the species in such numbers—more than a dozen in lifting a small piece of a rail—was an interesting discovery. In some communications contributed by me to some of the newspapers of Albany and Northern New York, I ascribed the above ravages to *Nephelodes violans.* Farther study led me to believe that I had been hasty in my reference.

Early in July, Mr. J. Q. Adams, of Watertown, N. Y., where the ravages of the same insect had also been observed, furnished me with information and material that convinced me that *N. violans* was only chargeable with a small portion of the above injuries, and that the principal depredator was the smaller larva observed and collected by me, which, from the cocoons forwarded to me at this time, undoubtedly belonged to the Pyralidæ. The cocoons were taken from the infested fields at Watertown, from just below the surface of the ground, where they were so numerous that a half dozen could be taken from a sod the size of a man's hand. On opening the cocoons, the larvæ were found lying within them, still unchanged, although they had been made over a month before, and they were identical with my Potsdam collections. Additional cocoons were opened by me early in July, when the larvæ were still in their untransformed state, in which they had at this time been remaining for from a month to a month and a half.

The delayed pupation is an interesting item in their history. It is known to occur in some of the Bombycidæ, among the Notodontas for example, when it extends over the winter, and the pupa state is assumed in the spring, a short time before the emergence of the perfect insect; but it was new to me at this season of the year.

Dr. Hagen, to whom I communicated the fact, was unable to find any record in the extensive library at Cambridge of such delayed pupation among the Pyralidæ, although Prof. Riley informs me that he had known of its occurrence in some of the species.

On the 6th of August, the first moth from the Watertown cocoons was disclosed, and it proved to be *Crambus vulgivagellus.* The interesting question as to which of our insect deprėdators was chargeable the ravages in Northern New York—more injurious in the extent of territory embraced than in an army worm invasion—was decided. The new enemy, the latest addition to our list of formidable insect pests, was found to be a modest, inconspicuous, hitherto unobtrusive little Crambus. It had long been known in our cabinets, but had never before presented itself as an injurious insect.

It is probable that several accounts of injuries to pasture lands, in New England States, during the last three or four years, which have been ascribed, either to the army-worm or an unknown depredator, are due to this

*Read before the A. A. A. S., Cincinnati, 1881.

species. In its subsequent appearance, hereafter, it may now be recognized.

The Crambidæ are small moths with narrow front wings, often marked with metallic spots and lines, which are frequently driven up for short staccato flights in our pastures and meadows during the fall months.

The paper concluded with a resumé of the history of the species, so far as known at present, which is omitted as not of general interest.

CAÑONS—THEIR CHARACTER AND ORIGIN.*

By Hon. William Bross.

To the professional geologist it may seem an impertinence for a layman to offer any opinions as to the character and the origin of cañons. He may, however, it is hoped, use his eyes without offense, and form such conclusions as the facts which he has observed, may appear to warrant. If they should not agree with the recognized principles of the science as now understood, he will be no worse off than scores of learned Professors in the past, for in this, as in almost every other science, nearly every conceivable absurdity was exhausted before theories were made to agree with acknowledged facts. And here, at the commencement, the conclusion to which the observations to be presented somewhat in detail have led, may as well be stated—viz.: that cañons were formed by some great convulsion of the earth's surface, or by the contraction of mountain chains from their igneous condition in the early history of the planet. Take, for instance, the cañon of the Saguenay—a vast fissure in the mountain chain that lies on the north side of and nearly parallel with the St. Lawrence. The fissure or cañon is some fifty or sixty miles long and lies nearly at right angles to the river. Something like a mile apart, the perpendicular rocks on the north side are, at some points, about 1,500 feet high, the water at their base being several hundred feet deep. No man in his senses, it seems to me, could possibly conceive that this gorge through the granite mountain could have been formed by the action of the insignificant river that empties into Ha-Ha Bay at the northern end of the cañon. The surface of the water, for the whole distance of sixty miles, is on a level with the St. Lawrence, in some places it is several hundred feet deep and the cañon is about a mile wide, through the solid granite rocks. And here another general principle may as well be stated, that, with a single exception, the width of this and the other cañons hereafter to be noticed, is scarcely ever more than a fraction of a mile; seldom a single mile—a fact that strongly indicates uniformity in their origin. And besides, the mountains on both sides are generally nearly of the same height.

TAKE THE CAÑON OF THE HUDSON,

where it passes through the Blue Ridge, above and below West Point. The channel is deep, the tide ebbing and flowing far upwards towards Albany; the mountains on both sides, though rounded off towards their summits, doubtless during the glacier period, are about of the same height, and there is a general correspondence in the dip and thickness of the vast strata of rocks on both sides of the river. With the exception that the cañon is far above tide water, the same general facts are witnessed in that of the Delaware at the water-gap through the same spur of the Allegheny Mountains. In this case there are two well-defined ledges corresponding with each other on both sides of the river; the water is deep and sluggish while passing through the gorge, and all the facts seem to point, with unerring certainty, to some great convulsion in Nature as the origin of the cañon. With the exception that the current of the Potomac is swift at Harper's Ferry, the break in the mountain there, so graphically described by Jefferson, is very similar to that of the Delaware. This gorge may not have been relatively as deep at its formation as those of the Hudson and the Delaware.

THE CAÑON OF THE NIAGARA

was confessedly formed by the action of the river; but, if the structure of the rocks forming the cañon between the falls and Lewistown be considered, the exception in this case, it is believed, will prove the rule enunciated at the beginning of this paper. The rocks underlying the country between Lewistown and Buffalo are nearly horizontal, and are, in round numbers, as indicated by the gorge below the falls, some 200 feet thick. The upper strata, for say half the distance, are solid limestone, underlaid for perhaps an unknown depth, by soft sandstone, scooped out with comparative ease by the great cataract. Hence, the support of the upper stratum of lime-rock is gradually worn away, and it falls into the gulf below. On the American side of Goat Island, where only a fraction of the river falls over the precipice, the lime-rock lies below in vast blocks, and a rapid is gradually forming, while on the Canada side, the immense river scoops out the sand-rock to a great depth, and the falling sections of the lime-rock are buried out of sight forever. Below the railway bridge, for a long distance, there is a terrible rapid, showing that some other rock at the bottom of the river was harder than the sandstone, or that the stream is partially damned up by the lime-rocks thrown down between the bridge and the present fall, forced to the position they now occupy by the water, débris, and ice pressing down from above as the river gradually receded towards Lake Erie. This recession will doubtless continue even back to Lake Erie, unless the sandstone dips deeper down into the earth, and the limestone strata become thicker or some other hard rock fills the entire face of the cataract. Then the fall would gradually wear away at the top and become a rapid of gigantic proportions. Now, if the Niagara River, with its vast volume of water at first falling over a lime-rock ledge, at Lewistown, underlaid by a friable sandstone base—a condition of things found, it is believed, in no other cañon upon the continent—has required untold ages to work its way up to its present location, how is it possible for the comparatively small rivers heretofore named, and those to follow, to wear away a pathway to the sea through great mountain ledges of the hardest rock? Such a conclusion would be absurd.

THE CAÑON OF THE MISSISSIPPI

extending, say from Dubuque to the head of Lake Pepin, some 200 miles or more, is an exception to the rule above proposed, mainly in its width, which is some five to seven miles. The sandstone bluffs on either side are generally perpendicular from the top downwards from 200 to 300 feet, when the débris slopes down to the bottom lands or to the majestic river as it sweeps through the alluvium from one side of this broad cañon to the other. There are doubtless good reasons for the opinion that the waters which now find their way from Lake Winnipeg to Hudson's Bay once flowed south and filled full the broad space between the beautiful bluffs of the Upper Mississippi.

THE GORGE OF THE UPPER MISSOURI,

situated about 100 miles below Fort Fenton, is one of the most marked, as it is one of the most beautiful, cañons on the continent. The walls are perpendicular, of white sandstone, scarcely a mile apart, and some eighty feet high. On the top of these walls there is a layer of clay, perhaps of the same thickness, rounded off gracefully by the winds and storms, while in some places it has been all worn away, and the tops of the white sandstone ledges appear as castellated forms, reminding one of the Milan Cathedral, or some of the old ruins

* Read before the A. A. A. S., Cincinnati, 1881.

scattered all over Europe. Between these sandstone walls the river flows smoothly, without giving the least suspicion that the cañon was formed by it. Only some great convulsion could have torn apart this immense sandstone deposit for some twenty or twenty-five miles. It will well repay a visit to the Upper Missouri to see it.

THE GRAND CAÑONS OF THE ARKANSAS,

the South Platte, Clear Creek and the Bowlder strongly resemble each other, and may, therefore, be disposed of in the same paragraph. Through the three first, in spite of the tremenduous obstacles they presented, railways have been built, and the saucy little locomotive rings out the echoes from their perpendicular granite walls on either side some 2000 to 3000 feet high. Small rivers—for they are small here—rush through them with angry roar; but it would be worse than idiotic to suppose that thev wore down the vast granite walls through which they run to the bed they now occupy. Only Nature's reserved forces, such as the world sees in earthquakes, could rend these granite mountains asunder, and, with perpendicular walls half a mile high, make a pathway for the tiny streams that surge and brawl between them.

THE CAÑON OF THE COLUMBIA

is, in some respects, one of the grandest upon the continent. From Cape Horn to perhaps some twenty miles or more above Celilo, at the head of the Dalles, a distance of sixty or seventy miles, the great river finds its way to the ocean through a gorge, the walls of which are from 500 to 4000 feet high. Even a cursory inspection will convince a practiced eye that for the entire height, and most of the distance, it is composed of nearly perpendicular basaltic rocks. No one series of columns where present reaches from the base to the top of the mountain; but at the foot of the cascades, on the south side of the river, their development is truly wonderful. Suppose before you there is a row of them 500 feet high and twice as long on the face of the mountain; at either end of that thousand feet another row, with their bases on a line with the tops of the first, shoots up another 500 feet, and so on from the base to the top, one row of columns above another, will convince the beholder that the entire mountain is composed of basalt. From the time of the Cæsars to the present all the world has been wondering at, or gazing with admiration at the Giant's Causeway, on the coast of Ireland. It, too, is composed of basaltic columns, and they are actually 300 feet high. Thus America furnishes to Great Britain a ratio in basalt of 3300 to 300; figures which I was wicked enough to write in 1879 would probably represent the influence of the two nations on the affairs of the world 100 years hence.

The Cascade Range, in Oregon and Washington Territory, corresponds with, and is virtually an extension of, the Sierra Nevada, in California. Near the western end of the cañon of the Columbia the Cascades form a splendid rapid, and the river falls thirty-five feet in two miles. From the head of the Cascades steamers run on the smoothly-flowing river for forty-five miles through the splendid cañon to the foot of the Dalles. Here, as the tourist glides along, Mount Hood, clad in a mantle of snow old as creation, peers down upon him through the lateral cañons, while the dark, frowning walls of basalt on either side almost make him shudder and forget for the moment how he can escape from this gloomy prison to the cheerful abodes of mankind. These stupendous basaltic walls, with the river flowing smoothly and beautifully between them, would never for a moment suggest the thought that this grand gorge was formed by the river. Only Nature herself, shaking as a reed this vast mountain-chain, could have rent it asunder and given us the sublime cañon of the Columbia. Only one other,

THE YOSEMITE VALLEY,

can be compared with it, and to that, as in some respects the grandest of them all, let us now turn our attention. A description of it will be most easily remembered by saying it is a gorge in one of the spurs of the Sierra Nevada Mountains, about twelve miles long, a mile wide, and a mile deep. As many, perhaps most of the members, have visited this grandest wonder of the world, only a brief description of it will be attempted. At El Capitan—or Tu-toc-a-nu-la, the granite wall—they are, on both sides, of the same material—is 3,300 feet high and very nearly perpendicular. At the grand arches, the height is about the same and the south dome is 6,000 feet—a very considerable fraction more than a mile—one-half of which is perpendicular. From either side the waterfalls are splendid. The Bridal Vail is 900 feet; the Yosemite, 2,634, more than half a mile; The Vernal Fall of the Merced River, at the head of the cañon, is 350 feet; and Nevada Fall is 700. The question is how was this vast gorge made through this mountain of granite? Prof. Whitney, if correctly reported, ascribes it to the dropping down towards the centre of the earth of a section of the mountain a mile wide. From this opinion, of this master of geological science, with all possible respect, I beg leave to differ. The facts of its structure, in my judgment, warrant the belief that, like all the other cañons above referred to—that of the Niagara alone excepted—it was formed by an upheaval of the mountain, at that particular point, sufficient to break it apart to the extent of a mile—the more probable cause; or the mountain, while intensely heated, contracted enough to do it. A few of my reasons for this opinion are as follows:

These solid granite mountains were once torn apart—on a smaller scale, it is true—for there are immense seams, perhaps two feet thick, of cream-colored feldspar, running through the walls of this valley; and it is believed that a correspondence can be observed in these seams on both sides of the gorge. If rent asunder to admit the injection of these seams of feldspar, why not on a larger scale? When this vast fissure was first made it was undoubtedly very deep, perhaps half a dozen miles or more. Where the break was in the line of the cleavage there the wall stands up perpendicularly, as at El Capitan, and the arches, and a few other points. Where it was not in the cleavage line, immense masses of rock were thrown into the abyss, and from this source and the débris brought down by the Merced river, the gorge gradually filled up to its present level. At El Capitan and the arches, the granite wall stands unbroken to the top, and you can ride right up to it and, from your saddle, put your hand on that wall rising sheer above you for more than three-fifths of a mile. Your horse stands on the fine disintegrated granite, the last contribution of the snowy range to the eastward. But after the valley was filled up to its present general level, at points where the cleavage was not in the line of the upheaval, as in the rear of Mr. Hutchins' hotel and some other places, the frost and perhaps earthquakes continued to throw down immense blocks, and hence there is at this point, a gradual slope to the top on the south side of the valley, with trees growing wherever they can find a ledge or a crevice to get root in. Another instance, showing how water, frost and other causes have broken the symmetry of the valley, may be seen at the Yosemite Fall. Both the height and the front of the escarpment, east and west of the Fall, are in the same line, while the ice and the stream have worn the wall back at the Fall perhaps a quarter of a mile from the front line. And yet the first perpendicular fall is 1,600 feet, or ten times the height of Niagara.

Such facts might be multiplied almost indefinitely, but

enough for this paper. This general remark, however, should be carefully weighed. The cañon of the Columbia, the Yosemite Valley, the Charquinez Strait connecting the Suisun and San Pablo Bays, and the Golden Gate itself, through which the waters of the Sacramento and the San Joaquin, draining the great Valley of California, find their way to the ocean, are all about a mile wide. With the exception of the cañon of the Mississippi, the same is true, it is here repeated, of all the cañons above referred to in the Rocky Mountains and east of them, noticed in this paper. It is submitted, therefore, that the main facts in regard to them, point almost unmistakably to a similar origin for them all. All these cañons I have myself visited, many of them several times. Several of them are splendid, even sublime, beyond the power of the most accomplished pen to describe. I dared not to attempt it, and have, therefore, simply stated what I have myself seen and drawn such conclusions as the facts seemed to warrant.

Let me add a very few words in conclusion upon a paper on the geological history of the Colorado River and the plateau of it, read at the St. Louis meeting by Col. E. C. Dutton, of Washington. This cañon, as described by Maj. Powell, who has the honor of braving almost incredible dangers to explore it and to give the world their first knowledge of its wonders, is some 1,500 miles long; the perpendicular walls are a mile or a fraction of it apart, and are from 1,000 to 5,000 feet high. They are composed of nearly all the series in the geological catalogue, from the granite all the way up to the highest igneous stratified rocks. Now this, by far the longest, and in some respects the most wonderful cañon in the world, Col. Dutton described as having been worn by the Colorado River. In view of the facts herein presented that conclusions seems supremely fanciful and absurd. Like all the others, it could only have been formed by some great convulsion of the earth's crust, and through it the drainage of nearly a thousand miles along the western slopes of the Rocky Mountains finds its way to the Gulf of California.

ADDRESS OF COL. GARRICK MALLERY, U. S. ARMY.

Chairman of the Subsection of Anthropology of the A. A. A. S. at the Opening of that Subsection.

THE GESTURE SPEECH OF MAN.

Anthropology tells the march of mankind out of savagery in which different people have advanced in varying degrees, but all started in progress to civilization from a point lower than that now occupied by the lowest of the tribes now found on earth. The marks of their rude origin, retained by all, are of the same number and kind, though differing in distinctness, showing a common origin to all intellectual and social development, notwithstanding present diversities. The most notable criterion of difference is in the copiousness and precision of oral speech, and connected with that, both as to origin and structure, is the unequal survival of gesture signs, which it is believed once universally prevailed. Where sign language survives it is, therefore, an instructive vestige of the prehistoric epoch, and its study may solve problems in philology and psychology. That study is best pursued by comparing the pre-eminent gesture system of the North American Indians with the more degenerate or less developed systems of other people.

EXAMINATION OF THE INDIAN SYSTEM.

The conditions and circumstances attending the prevalence, and sometimes the disuse, of sign language in North America were explained. The report of travelers that among Indians, as well as other tribes of men, some were unable to converse in the dark, because they could not gesture, is false. It is the old story of Βάρβαρος and ἀγλασσος applied by the Greeks to all who did not speak Greek, repeated by Isaiah of the "stammering" Assyrians, and now appearing in the term *slav* (speaker arrogated to themselves by a large division of the Aryan family), as contradistinguished by the Russians from the Germans, whom they stigmatize as *Njemez* (speechless.)

The theory that sign language was the original utterance of mankind does not depend upon such tales or prejudices. After the immeasurable period during which man has been upon the earth it is not probable that any existing peoples can be found among whom speech has not obviated the absolute necessity for gesture in communication between themselves.

The assertions made that the sign language of Indians originated from one definite tribe or region supposes its comparatively recent origin, whereas the conditions favorable to its development existed very long ago and were co-extensive with the territory of North America occupied by any of the tribes. Numerous evidences were presented as to its antiquity and generality. But the signs are not now, and from the nature of their formation never were, identical and uniform. The process is the same as among uninstructed deaf mutes when associated together, which was explained.

A comparison sometimes made of the diversities of the sign language of the Indians with the dialects and provincialisms of the English language is incorrect, as there is so small a proportion of the sign-using tribes which make identically the same signs to express the same ideas, and also because the signs are not absolute and arbitrary as are the words of English.

ARE SIGNS CONVENTIONAL OR INSTINCTIVE?

Sign language, as a product of evolution, has been developed rather than invented, but each of the separate signs had a definite origin arising out of some appropriate occasion, and the same sign may thus have had many different origins due to identity in the circumstances. No signs in common use were at first conventional. What may appear to be convention largely consists in the differing forms of abbreviation which have been adopted. Yet, while all Indians, as well as all gesturing men, have many signs in common, they use many others which have become conventional in the sense that their etymology and conception are not now known or regarded by those using them. The conventions by which such signs were established occurred during the long periods and under many differing circumstances. Our Indians, far from being a homogeneous race and possessing uniformity in their language, religions and customs, differ from each other more than do the several nations of Europe, and their semiotic conceptions have correspondingly differed.

PERMANENCE OF SIGNS.

Instances were presented of the ascertained permanence of some Indian signs, and of those of foreign peoples and deaf mutes. Though they, as well as words, animals and plants, have had their growth, development and change, those which are general among Indian tribes, and are also found in other parts of the world, must be of great antiquity. Many signs but little differentiated were unstable, while others that have proved to be the best modes of expression have survived as definite and established.

IS THE INDIAN SYSTEM SPECIAL AND PECULIAR?

The Indian system as a whole was compared with those of foreign peoples—the ancient Greeks and Romans, the modern Italians, the Turks, Armenians and Koords, the Bushmen of Africa, the Redjangs and Lelongs of Sumatra, the Fijians, the Chinese, Japanese, and the Australians. The result is that the so-called sign language of Indians is not, properly speaking, one language, but that it and the gesture systems of deaf-mutes and of all

peoples constitute together one language, the gesture language of mankind, of which each system is a dialect. The generic conformity is obvious, while the occasion of specific varieties can be readily understood.

ARCHÆOLOGIC RELATIONS.

The most interesting light in which Indians, as other lower tribes of men, are to be regarded, is in their present representation of the stage of evolution once passed through by our ancestors. Their signs, as well as their myths and customs, form a part of the paleontology of humanity. Their picture writings are now translated by working on the hypothesis that their rude form of graphic representation, when at the same time a system of ideographic gesture signs prevailed, would probably have been connected with the latter. Traces of the signs now used by the Indians are also found in the ideographic pictures of the Egyptian, Chinese and Aztec characters.

HISTORY OF THE GESTURE LANGUAGE.

From the records of the ancient classic authors, and also from the figures on Etruscan vases and Herculanean bronzes and other forms of Archaic art, it is certain that a system of gesture language is of great antiquity. Later, Quintilian gave elaborate rules for gesture, which are specially noticeable for the significant disposition of the fingers still prevailing in Naples. The ancient and modern pantomimes were discussed, and also the gestures of speaking actors in the theatres, the latter being seldom actually significant or self-interpreting even, in the expression of strong emotion. The same scenic gesture must apply to many diverse conditions of fact. Its fitness consists in being the same which the hearer of the expository words would spontaneously assume, if yielding to the same emotions, and which, therefore, by association, tends to induce sympathetic yielding. But the communication of the facts themselves depends upon the words uttered. A true sign language would express the exact circumstances, with or without any exhibition of the general emotion appropriate to them.

PRACTICAL APPLICATION OF SIGN LANGUAGE.

This was shown to be in successful use in cases cited by travelers skilled in it, and its powers were compared with those of speech. It finds actually in nature an image by which any person can express his thoughts and wishes on the most needful subjects to any other person. Merely emotional sounds may correspond with merely emotional gestures, but whether with or without them would be useless for the explicit communication of facts and opinions of which signs themselves are capable. Notwithstanding frequent denials, they are able to express abstract ideas. The rapidity of their communication is very great, and can approach to that of thought. Oral speech is now conventional, and with the similar development of sign language, conventional expressions could be made with hands and body more quickly than with the vocal organs, because more organs could be worked at once.

But such rapidity is only obtained by a system of preconcerted abbreviations and by the adoption of absolute forms, thus sacrificing self-interpretation and naturalness, as has been the case with all oral languages in the degree of their copiousness and precision.

RELATIONS IN PHILOLOGY.

Signs often gave to spoken words their first significance, and many primordial roots of language are found in bodily actions. Examples are given of English, Indian, Greek and Latin words in connection with gesture signs for the same meaning, and the structure of the sign-language was compared with the tongues of this continent, with reference also to old Asiatic and African languages, showing similar operations of conditions in the same psychologic horizon.

ORIGIN OF SPEECH.

It is necessary to be free from the vague popular impression that some oral language of the general character of that now used by man is "natural" to man. There is no more necessary connection between ideas and sounds, the mere signs of words that strike the ear, than there is between the same ideas and signs for them which are addressed only to the eye. Early concepts of thought were of a direct and material character. This is shown by what has been ascertained of the radicals of language, and there does not seem to be any difficulty in expressing by gesture all that could have been expressed by those radicals.

CONCLUSIONS.

It may be conceded that after man had all his present faculties, he did not choose between the adoption of voice and gesture, and never with those faculties, was in a state where the one was used, to the absolute exclusion of the other. The epoch, however, to which our speculations relate is that in which he had not reached the present symmetric development of his intellect and of his bodily organs, and the inquiry is: Which mode of communication was earliest adopted to his single wants and informed intelligence? With the voice he could imitate distinctively but few sounds of nature, while with gesture he could exhibit actions, motions, positions, forms, dimensions, directions and distances, with their derivations and analogues. It would seem from this unequal division of capacity that oral speech remained rudimentary long after gesture had become an efficient mode of communication. With due allowance for all purely imitative sounds, and for the spontaneous action of vocal organs under excitement, it appears that the connection between ideas and words is only to be explained by a compact between speaker and hearer which supposes the existence of a prior mode of communication. This was probably by gesture. At least we may accept it as a clew leading out of the labyrinth of philological confusion, and regulating the immemorial quest of man's primitive speech.

TRICHINÆ CYSTS.

The mode of formation of the cyst of trichina has been studied by M. Chatin and described in a communication to the Académie de Sciences. It was formerly said to be formed partly from the contractile tissue, and partly by a secretion from the nematoid, but this opinion was based only on some apparent differences in the thickness or aspect of the cyst wall, and not on any careful study of its formation, which necessitates the examination of animals dying or killed in different states of the affection. When it arrives in the muscles the worm forms adhesions with the interfascicular tissue in which rapid changes occur. The elements increase in size, and during the growth of the protoplasm it assumes the appearance of an amorphous mass, in which, however, nuclei and vacuoles can be seen, which seem to indicate that the mass consists really of aggregated cells. By the growth of this the primitive fibres are compressed. In the new protoplasm fine proteoid granulations are first observed, and then other granulations which present all the reactions of glycogen. Then follow important changes in the periphery of the granular mass, containing the trichina, now curled up in the interior; the outer surface becomes distinctly thickened and indurated, and may then become lamellated or present granulations or folds. The sarcolemma takes no part in the formation of the cyst except occasionally furnishing it with a purely adventitious layer. Moreover, when the nematoid contracts its first adhesions to sarcolemma, and not to the interfascicular tissue. it rapidly dies without determining a new formation.

BEFORE leaving New York, King Kalakaua called on Mr. Edison. He was accompanied on his visit by the Attorney-General of his island kingdom, Mr. Armstrong, and by an intimate friend residing in this city, whose acquaintance he made in Vienna. Punctually at nine o'clock in the evening his majesty alighted from the carriage of his friend in front of the Fifth-avenue mansion. He was introduced to Mr. Edison, who escorted him through the building, and by means of models, maps, drawings, and the 55 lamps in operation, explained the theory of the conversion of steam-power into electricity and the generation of light in the carbon loop.

Escorting his distinguished visitor to the library, Mr. Edison first explained the science of the light, and then, by reference to maps of the district that his engineers are preparing for the experiment, the application of his system to the practical requirements of a city. The region to be lighted will require 22,000 lights, all of which are to be supplied from a central station in Pearl street, where 12 engines of 185 horse power are to be placed. Ten of these will be in constant operation, the other two being held in reserve to meet the emergencies of accident. The engines will be run at a rate of speed equal to that of a locomotive at 60 miles an hour, and a new feature of the system is that no belts are employed to transmit the power to the dynamo-electric generators, and the power is applied directly, avoiding the irregularity and vibration arising from the slipping of the belt which seems inseparable from the old practice. The mains consist of large iron pipes, in which the crescent-shaped positive and negative conductors are carried, being insulated from such by means of a non-conducting material with which the pipes are filled when in a pasty condition, induced by heat, but which hardens like a concrete pavement in the process of cooling. These mains in their passage through the streets are all connected with each other by means of ingenious connection boxes, the whole forming a subterranean net-work of electrical conductors comparable to the capillary circulation in the skin of an animal body.

His Majesty listened with intense but almost silent interest, and examined the cross-sections of the electrical mains and the interior arrangement of the connection boxes with critical closeness, now and then asking a question in the purest English imaginable, and with a voice that was strikingly low, mellow, and muscial, and yet so sharply defined in the articulation of the consonants as to impress the ear at an unusual distance. He seemed particularly interested in the statement that after steam power had been transformed into electricity and carried to a great distance in that form it could again be converted into motive power by means of an electrical motor, and sold to customers for the purpose of running elevators or operating hoist-ways. His eyes lighted when he was told that one of the most profitable departments of the business of the company would be the sale of power to manufactories and business firms in quantities as small as a single horse-power, costing, under circumstances of ordinary use, not more than eight cents a day.

From the library Mr. Edison led the way to the front parlor, brilliantly lighted. Pressing the toe of his shoe upon a knob projecting from the floor, every lamp was instantaneously extinguished and as suddenly blazed out again. The inventor next turned the stop-cock of a single lamp among the group and extinguished it. The party then ascended to the upper floor, where more wonders were in store, and then descended two flights beneath the street level, where, in a low-ceiled vault, a small engine was operating, with nearly absolute silence, a generator whose cylinder performed 1200 revolutions per minute. After inspecting every detail, his Majesty took leave of the inventor, and repaired to his carriage. One of the points that appeared to impress him most was the steadiness of the light, and its freedom from vibration.—*N. Y. Times.*

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING SEPT. 24, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

SEPTEMBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 18..	30.094	30.228	0 a. m.	29.964	12 p. m.	68.3	60.6	78	3 p. m.	64	5 p. m.	53	6 a. m.	53	6 a. m.	142.
Monday, 19..	29.831	29.964	0 a. m.	29 788	4 p. m.	75.0	65.3	84	3 p. m.	70	6 p. m.	64	6 a. m.	60	6 a. m.	140.
Tuesday, 20..	29.869	29.898	9 a. m.	29.810	5 p. m.	73.3	63.3	83	4 p. m,	69	4 p. m.	63	6 a. m.	58	7 a. m.	134.
Wednesday, 21..	29.999	30.068	12 p. m.	29.892	0 a. m.	60.7	59.3	73	4 p. m.	64	4 p. m.	62	12 p. m.	56	7 a. m.	130.
Thursday, 22..	30.069	30.108	10 a. m.	30.012	5 p. m.	66.6	62.3	70	4 p. m.	65	4 p. m.	60	5 a. m.	59	5 a. m.	87.
Friday, 23..	29.940	30.046	0 a. m.	29.882	4 p. m.	74.6	67.3	85	4 p. m.	70	2 p. m.	67	6 a. m.	64	6 a. m.	139.
Saturday, 24..	29.985	30.004	9 a. m.	29.942	0 a. m.	77.3	68.3	85	4 p. m.	73	4 p. m.	68	6 a. m.	64	6 a. m.	141.

Mean for the week .... 29.969 inches.
Maximum for the week at 0 a. m., Sept. 18th .... 30.228 "
Minimum " at 4 p. m., Sept. 19th .... 29.788 "
Range .... .440 "

	Dry.		Wet.	
Mean for the week	71.7	degrees	63.7	degrees.
Maximum for the week, at 4 pm. 24th	85.	" at 4 pm 24th,	73.	"
Minimum " " 6 am. 18th	53.	" at 6 am 18th,	53.	"
Range " "	32.	"	20.	"

SEPTEMBER	WIND. Direction. 7 a. m.	Direction. 2 p. m.	Direction. 9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Force. Time.	HYGROMETER. Force of vapor. 7 a. m.	Force of vapor. 2 p. m.	Force of vapor. 9 p. m.	Relative humidity. 7 a. m.	Relative humidity. 2 p. m.	Relative humidity. 9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Begining.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0–10
Sunday, 18.	n. n. e.	n.	n. e.	60	¼	9.00 am	.380	.436	.482	76	48	66	0	1 cir. s.	4 cir. cu.	-----	-----	-----	--	2
Monday, 19.	n.	n. n. e.	n.	130	1½	12.00 m	.425	.447	.628	64	39	72	2 cir. cu.	2 cir. s.	5 cu.	-----	-----	-----	--	8
Tuesday, 20.	n. w.	n. n. w.	w.	90	1¾	2.00 am	.403	.438	.519	67	41	60	0	2 cir. cu.	3 cir. cu.	-----	-----	-----	--	8
Wednesday, 21.	n. e.	e. s. e.	s. s. e.	152	1¼	7.30 am	.356	.390	.483	62	50	78	0	0	0	-----	-----	-----	--	0
Thursday, 22.	s. s. e.	s.	s. s. w	153	2¼	7.20 pm	.447	.529	.543	77	74	79	3 cir. cu.	10	10	-----	-----	-----	--	0
Friday, 23.	w. s. w.	s. w.	w. s. w.	236	7	1.40 pm	.556	.558	.604	84	49	72	10	6 cir. cu.	0	-----	-----	-----	--	0
Saturday, 24.	w.	s.	s. s. w.	129	¾	3.30 pm	.529	.583	.612	75	50	62	1 cir.	4 cir. cu.	0	-----	-----	-----	--	0

Distance traveled during the week .... 950 miles.
Maximum force .... 7 lbs.

Total amount of water for the week .... .00 inch.
Duration of rain .... 00 hours, 00 minutes.

DANIEL DRAPER, Ph. D.

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